ROYAL ASTRONOMICAL SOCIETY.

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G. B. AIRY, Esq., President, in the Chair.

Follett Osler, Esq., Hazelwood, near Birmingham, and the Rev. Stephen Parkinson, St. John's College, Cambridge,

were balloted for and duly elected Fellows of the Society.

Letter from Mr. Lassell to the Rev. R. Sheepshanks.

"I have now to trouble you with the results of my observations upon *Uranus* and his satellites, which have now got too much into the twilight for further observation. In another mail or two I may probably send you a summary of my observations upon *Saturn*, with some more diagrams, recording in detail some of his delicate phenomena, with the dates in order of time. It will include also a series of observations of *Hyperion* and some measures of the diameters of the planet and major axis of his ring. From its voluminous appearance, however, amounting to three or four sheets, I feel some diffidence about sending it off, though I cannot but persuade myself that it may be an interesting record to refer to, of the appearance of an object which is unique and of such surpassing interest.

"I will mention *en passant* a phenomenon of the obscure ring which I have only lately noticed, and which I fear cannot much longer be observed during this apparition: indeed, the sky during the last month, and thus far in the present one, has yielded me only a small fraction of the fine nights I have been used to enjoy in the same period here. The obscure ring appears to me to vary in colour, one end of it being often bluish-gray, while the other is brown. For instance,

Dec. 28	preceding end		bluish-gray	following	brown
Jan. 6	—	—	gray	—	brown
12	—	—	brown	—	gray
25	south preceding		gray	north following	brown
28	—	—	brown	—	— gray
Feb. 3	north preceding		brown	south following	gray

" The sky has not permitted me to pursue this very delicate observation so as to arrive at any conclusion about the rotation of the ring; but there is no question about the fact of a difference of colour being sometimes observable.

" I am now beginning to look towards the time of my departure, which I fancy will be about the middle of next month. I don't think we shall leave the island without some regrets for the loss of its splendid skies and marine views, and also for the loss of the agreeable society we have enjoyed here.

" *Observations of the Satellites of Uranus made with the 20-foot Equatoreal at Valetta.*

	Ariel.			Umbriel.		
	Position.		Distance.	Position.		Distance.
1852. G.M.T.	h m	o ′	h m ″	h m	o ′	h m ″
Oct. 15	...	310 0	10 0 10	...	294 0	... 14
16	...	138 0	... ...	...	175 0	... ...
18	11 30	210 0	11 30 ...	11 30	12 0	... . .
29	9 0	...	... ...	...	...	... . .
30	9 24	330 40	... 15	9 45	58 59	... 18
31	...	190 0	... 14	...	350 0	... 18½
Nov. 1	...	130 0	... ...	...	5 0	... ...
3	...	113 0	... 10	...	89 0	... 14
4	9 47	331 34	10 21 12·03	9 39	352 35	10 8 20·70
6	9 26	32 48	... ...	9 36	173 35	... ...
10	9 23	188 40	... 11½	9 23	186 40	... 19¾
11	...	44 0	... ...	...	99 0	... ...
12	...	265 0	... ...	...	20 0	... ...
15	9 0	190 56	... 13	0	...	... 19
16	...	52 0	... ...	...	22 0	... ...
18	...	135 0	... 13¾	...	211 0	... 16½
19	...	344 0	... ...	...	146 0	... ...
20	9 10	193 38	... 13¾	8 59	38 44	... 21
Dec. 6	...	93 0	... ...	...	...	... ...
7	...	300 0	... ...	...	...	... ...
8	...	283 0	... ...	...	173 0	... ...
9	8 9	3 58	8 54 13·64	8 28	186 41	9 12 17·43
13	8 14	164 58	8 27 13·18	8 20	203 24	8 38 17·12
1853. Jan. 7	...	...	5 57 13·58	...	...	5 46 15·96
11	6 5	348 22	... ...	6 11	210 0	... ...

1852.	Titania. Position. G.M.T. h m	° ′	Distance. h m	″	Oberon. Position. h m	° ′	Distance. h m	″
Oct. 15	...	168 46	...	30·38	...	332 15	...	40·86
16	10 18	131 55	10 38	27·91	10 10	305 17	10 30	37·85
18	11 41	34 32	12 1	28·76	11 34	239 0	11 50	34·90
29	9 18	317 35	9 37	28·46	9 10	321 52	9 24	40·48
30	10 7	265 8	10 28	25·17	9 56	289 10	10 21	36·19
31	9 50	218 27	10 14	27·39	9 41	256 26	10 1	33·24
Nov. 1	9 50	182 54	10 9	32·44	9 43	224 39	9 59	37·93
2	12 17	146 25	12 32	29·95	12 12	206 40	12 24	42·36
3	9 12	108 46	9 38	25·48	9 2	178 26	9 26	43·19
4	9 29	57 58	10 35	25·62	9 18	157 19	10 30	42·84
5	8 46	17 5	9 3	30·33	8 38	134 48	8 55	38·29
6	9 14	342 14	10 10	31·56	9 6	104 43	9 47	33·58
10	9 11	172 7	9 47	33·08	9 1	353 50	9 31	43·79
11	9 31	138 39	9 50	28·71	9 25	330 23	9 40	42·21
12	8 51	93 51	9 15	25·13	8 43	306 19	9 3	37·94
15	8 51	332 29	9 29	31·19	8 42	211 50	9 15	38·33
16	9 0	291 47	9 30	25·83	8 46	187 26	9 11	43·57
18	9 2	195 45	9 24	30·02	9 3	145 10	9 13	41·43
19	8 51	163 45	9 20	32·17	8 42	118 59	9 4	36·23
20	8 47	129 19	9 30	27·47	8 36	88 20	9 22	34·05
Dec. 6	8 5	178 17	8 28	32·15	7 54	14 22	8 37	41·58
7	8 22	144 29	8 51	30·37	8 13	350 37	8 36	43·68
8	8 36	102 48	8 53	24·95	8 24	328 54	8 42	41·26
9	8 38	50 2	...	...	8 37	304 2	...	...
13	9 0	245 21	8 50	24·54	8 57	184 14	8 43	42·02
1853. Jan. 7	6 40	306 43	6 28	27·74	6 30	238 8	...	...

"*Remarks.*

"Oct. 15. Being the first occasion on which I measured the satellites here, I was greatly struck with their brightness, even the close ones scarce required looking for, and all were well seen with the planet full in the field. *Oberon* the brightest; *Titania* slightly inferior; *Ariel* much brighter than *Umbriel*, which is rather less favourably situated.

"Oct. 16. The planet appearing as a bright sixth-magnitude star, and easily recognisable, I set the telescope upon him for the first time by the naked eye, without using the circles. The close satellites fainter than yesterday.

"Oct. 18. *Titania* decidedly brighter than *Oberon*. Measures of distance taken with difficulty, the sky being variable and often slightly clouded.

"Oct. 29. Only one of the close satellites seen. The nearly full moon within one hour and a quarter of the planet, and more north in declination, puts out the small satellites and renders it not very easy to observe the bright ones.

"Oct. 30. No measures of distance of the faint satellites could be got, the measures of position being, indeed, difficult in the bright moonlight distance of *Ariel*, estimated at six-tenths of the distance of *Titania*; distance of *Ariel* estimated to that of *Umbriel* as five to six.

"Nov. 2. Troubled by thin clouds, which interrupted the observations and prevented more than three measures of each being taken. Close satellites not seen.

" Nov. 4. Atmosphere unusually fine. All the satellites, even the faint ones, measured in position and distance with remarkable ease. *Titania* rather brighter than *Oberon;* measuring power on the micrometer 760, though the satellites were brightest and, on the whole, best seen, with 1018 negative eye-piece. Under these favourable circumstances, I surveyed the planet round to the distance of $2\frac{1}{2}'$ without finding any points of light which might be satellites. Even the faint satellites with this power are bright objects, and not for a moment to be overlooked. The disk of the planet harder and sharper than I have ever seen it before. With 1917 the faint satellites are, I think, even more obvious.

" Nov. 5. *Titania* brighter than *Oberon,* and, therefore, easier to measure. The close satellites not well seen to-night.

" Nov. 6. Interior satellites too faint for measurement in distance.

" Nov. 10. The close satellites in a line with the centre of the planet at $8^h\ 30^m$ G.M.T. *Ariel* three-tenths and a half of the distance of *Titania; Umbriel* six-tenths of the same distance from the planet. Both too faint for measures of distance. *Oberon* brighter than *Titania.*

" Nov. 12. Air unfavourable. Measures taken with much difficulty.

" Nov. 15. Wind troublesome, setting right in the opening of the roof; and the close satellites, though plainly seen with 1018, could not be satisfactorily measured in distance. *Ariel* very much brighter than *Umbriel.* Distance of *Ariel* estimated one-third that of *Oberon. Umbriel* about half the distance of *Oberon.* No other stars visible within $3'\frac{1}{2}$ of the planet, with power 1018 (except some which were *ascertained* to be stars).

" Nov. 16. *Umbriel* appears to-night brighter than *Ariel,* which is near its closest approach to the planet.

" Nov. 20. Satellites singularly placed, describing an almost perfect parallelogram.

" Nov. 22. The sky being unfit for measurements of the satellites, I measured the diameter of the planet. The mean of eight measures with the ordinary micrometer power of 760, gave $4''{\cdot}128$.

" Dec. 6. *Titania* in almost the best possible position for measures of distance; and, accordingly, the extreme difference of six measures amounts only to $0''{\cdot}39$. *Umbriel* not seen.

" Dec. 7. Interrupted by clouds; and though the faint satellites had been so bright as to be easily discovered by a non-astronomical friend, who happened to be present, and who had no knowledge of their places, no measures of them were obtained.

" Dec. 8. The disk of the planet very well defined and sharp with 1018; but no ellipticity nor any markings upon his surface can be observed.

" Dec. 9. Obtained measures in position and distance of the close satellites with power 760. Also measured diameter of planet with the same power; the mean of eight measures gave $4''{\cdot}097$.

" Dec. 13. Measured the four satellites in position and distance with 760. *Oberon* considerably brighter than *Titania.* By hiding the planet behind one of the bars of the micrometer, all four satellites were visible at once with 297, the inner ones with surprising brightness, taking into account their extreme proximity to the planet with that power. They instantly vanished when the planet was exposed. The satellites being favourably situated, this experiment was made to ascertain with how small a power they could be seen; and I think this is about the minimum for the close ones.

" Dec. 20. In a very fine sky I was struck with the beauty of the disk of *Uranus* with 297. In scrutinising his surface I frequently suspected a spot near his centre, but could not in the end be certain whether it was real or an optical illusion. Surveyed the planet with 565, with which the bright satellites were instantly seen, notwithstanding the very close neighbourhood of the moon and a rather hazy sky. The field was, however, so full of light, that it would have been in vain to look for the faint ones. The edge of the disk was as hard and sharp with this power as I have often seen the edge of *Jupiter* with a fine refractor of three or four inches' aperture. It was so fine that I applied a power of 778, which it bore admirably; but I could not verify the suspected spot nor

satisfy myself that any phenomena were visible upon his surface; nor that there was any sensible ellipticity of form. I seized the opportunity of measuring his disk; power 760. The mean of eight measures gave 4″·466.

" Dec. 22. The mean of eight measures of the planet's diameter with power 1027 gave 3″·945. Sky rather less favourable than usual.

" Dec. 27. The mean of nine measures of the disk with 760 gave 4″·032.

" 1853, Jan. 7. All the satellites well seen with 565, and still better with 1018, which, under good atmospherical circumstances, I generally find the most efficient power for such scrutiny. During this series of observations, though I have very commonly seen small fixed stars in the field with the satellites, I have never seen any which, by magnitude and position, could ever be mistaken for them. I find this peculiarity attend the close satellites, that they do not bear illumination well, and, therefore, cannot be ordinarily seen when the moon is near enough sensibly to brighten the sky. Though very conspicuous objects in the large dark field of a negative eye-piece, the faintest illumination greatly reduces their brightness; and to measure them micrometrically is almost always a work of labour and difficulty.

" Jan. 11. Surveyed *Uranus* with 1018 under a fine sky and admirable definition, the disk appearing perfectly round, and having a remarkably hard and sharp edge. On this occasion, as well as many others which I have not thought it necessary to particularise, I carefully scrutinised the neighbourhood of the planet to the distance of 5′ from his centre, for the discovery of other satellites. In the course of this scrutiny I made many measurements and diagrams of the positions of small points of light, which all turned out to be stars; and I cannot now resist the conviction, amounting, indeed, in my own mind to certainty, that *Uranus* has no other satellites visible with my eye and optical means. In other words, *I am fully persuaded that either he has no other satellites than these four, or if he has, they remain yet to be discovered.*

" The most probable periods of the newly discovered satellites which I have been able to deduce from this series of observations, combined with that of 1851, and the single observation of Nov. 6th, 1847, are, for *Ariel* 2·520378 days, or $2^{d}\ 12^{h}\ 29^{m}\ 20^{s}\cdot66$, and for *Umbriel* 4·144537 days, or $4^{d}\ 3^{h}\ 28^{m}\ 8^{s}\cdot00$. The period of *Umbriel* is almost identically the same as that given in the *Monthly Notice* for March last, page 154, from the observations of 1851; and the difference in that of *Ariel* admits of easy explanation. Owing to the shortness of the period of this satellite, and my being only able to get *estimations* of the position-angles in 1851, and further, having no certain observation to serve as an epoch later than 1847, I could not, from the observations of 1851 alone, ascertain the period so exactly as to be *sure* of the *number* of revolutions elapsing in those four years. In my determination of the period last year I concluded that, in the interval between 1847, Nov. 6·429, and 1851, Nov. 2·500, 580·114 revolutions had occurred, which gave a period of 2·511700 days. It now appears that only 578·114 revolutions could have taken place in that interval, giving a period of 2·520388 days, extremely near to that which satisfies best all the observations.

" Wm. Lassell.

" *Valetta, 8th Feb.* 1853."

Note on the Form of the Planet Saturn. By the Rev. R. Main.

My object in offering a few remarks on this subject, is to obviate any erroneous impressions which might arise from the publication, through mistake, of a paragraph of a letter from Mr. Dawes to Mr. De La Rue, in which doubts are expressed as to the correctness of the resulting ellipticity of the planet deduced by me in a paper printed in the 18th volume of the *Memoirs* of this Society. I have the authority of Mr. Dawes for stating that this portion of his letter was not intended for publication, and that his remarks on my result would have been very much modified if it had been so.

Since, however, any opinion expressed by so accomplished an observer must necessarily carry great weight with it (and in this instance a weight which he would probably not desire), I have thought it right to bring before the notice of the Society a few circumstances which additional research has brought to light, and which have not only confirmed me in my idea of the general correctness of my result, but which make it evident to *me* that it can scarcely be wrong by any significant quantity.

During the time of the visibility of the ring of *Saturn*, the effect of its vicinity to the equatoreal diameter produces so much confusion of the images in the use of a double-image eye-piece micrometer, or of a heliometer, that I doubt whether any trustworthy results could be at all produced, or at all events any results comparable with those deduced from the measures of a planet, without any such obstruction. I have never used a wire-micrometer for measuring *Saturn*, and I therefore do not profess to judge of the accuracy which might be practicable with it under similar circumstances; but my own impression is that the ring would prevent very great delicacy or accuracy in the measures. I, however, lay it down as a first principle, that *all measures made with a double-image apparatus capable of producing a definitive result for the ellipticity of the planet must be made during the time of the invisibility of the ring.*

When my paper was printed, I was actually ignorant of the existence of any extensive series of measures made for determining the form of *Saturn*, and this will be evident to any one reading the remarks in my introduction; and it was some time afterwards before my attention was directed to an excellent series made by Bessel with the Königsberg heliometer in the years 1830–1833, and printed at pp. 169, 170 of the 12th volume of the *Astronomische Nachrichten*. These measures were made with the same object as my own, viz. for the determination of the general form of the planet, as well as of the correct value of the ellipticity. None of them, however, were made during the time of the *total* disappearance of the ring, which took place for the first time at the end of September 1832. The ring reappeared in December 1832, vanished a second time near the end of April 1833, and finally re-

appeared in June of the same year. The only observations recorded by Bessel during this period are included between 1832, Feb. 9 and March 31, and 1833, Feb. 22 and March 29, and during this interval the ring was so small that Bessel desisted from measures of it "because," to use his own words, "the small breadth of it did not appear advantageous for the purpose;" it was, therefore, a very proper time for determining with the heliometer the ellipticity of the planet. On three nights, viz. on Feb. 20, and March 1 and 31 of 1832, he measured the planet in five directions, and convinced himself of its elliptical shape: but we are not concerned with these measures in our present inquiry.

The final results given by Bessel, as deduced from the whole series of measures, and reduced to the mean distance of *Saturn* from the earth (whose log he takes to be 0·9796480), are as follows:—

Diameter of ring.....	$=39''\cdot311$	from 44	observations
Equatoreal Diameter of *Saturn*	$=17''\cdot053$	70	—
Polar Diameter	$=15''\cdot381$	68	—

Hence the ellipticity is $\frac{1}{10\cdot2}$; and this is given by Mr. Hind and others as his definitive result.

If, however, we take the measures made when the ring had nearly disappeared, we shall find a result agreeing almost exactly with my own, and therefore preventing even that trifling doubt which arises from the difference between the above result and my own, $\frac{1}{9\cdot2}$.

I will for the sake of clearness give these measures, which are, when reduced to the mean distance, as follows:—

	Equatoreal Diam.	Polar Diam.	Difference.	No. of Measures.
	$''$	$''$	$''$	
1832, Feb. 9	16·89	15·03	1·86	2
11	16·97	14·87	2·10	2
13	16·91	14·95	1·96	1
14	16·94	15·20	1·74	2
15	17·18	15·23	1·95	2
17	17·25	15·28	1·97	2
20	17·22	15·24	1·98	1
March 1	17·00	15·35	1·65	3
11	17·12	15·45	1·67	3
16	16·93	...	...	
31	17·25	15·34	1·91	3
1833, Feb. 22	16·97	15·06	1·91	3
23	17·07	15·23	1·84	3
	16·95	15·18	1·77	3
27	17·14	15·07	2·07	3
March 29	17·26	15·36	1·90	3

The separate ellipticities deduced are as follows :—

1832, Feb.	9	Ellipticity = $\frac{1}{9\cdot08}$	= 0·11013
	11	— $\frac{1}{8\cdot08}$	= 0·12376
	13	— $\frac{1}{8\cdot63}$	= 0·11587
	14	— $\frac{1}{9\cdot74}$	= 0·10267
	15	— $\frac{1}{8\cdot81}$	= 0·11351
	17	— $\frac{1}{8\cdot76}$	= 0·11416
	20	— $\frac{1}{8\cdot70}$	= 0·11494
March	1	— $\frac{1}{10\cdot30}$	= 0·09709
	11	— $\frac{1}{10\cdot25}$	= 0·09756
	31	— $\frac{1}{9\cdot03}$	= 0·11074
1833, Feb.	22	— $\frac{1}{8\cdot88}$	= 0·11261
	23	— $\frac{1}{9\cdot28}$	= 0·10776
		— $\frac{1}{9\cdot58}$	= 0·10438
	27	— $\frac{1}{8\cdot28}$	= 0·12077
March	29	— $\frac{1}{9\cdot08}$	= 0·11013

Taking the simple mean of the above results, which will be quite accurate enough for our present purpose, we get, for the definitive ellipticity deduced by Bessel by observations made when the ring had nearly disappeared, 0·11041, or $\frac{1}{9\cdot06}$, a result agreeing as closely with the value deduced by me as could be expected.

In the eleventh volume of the *Nachrichten*, No. 264, pp. 421, 422, I find some measures made by Dr. Hussey with a double-image micrometer, for the determination of the ellipticity of *Saturn*, during the time of the disappearance of the ring, extending from 1833, April 30, to May 20, when, as he remarks, "the planet was in the most favourable position for accuracy of micrometrical measurement." His final result for the diameters of *Saturn* at the unit of distances is,

$$\text{Equatoreal Diameter} = 2'\,56''\cdot35$$
$$\text{Polar Diameter} = 2\;40\cdot21$$

$$\text{Hence the ellipticity} = \frac{16\cdot14}{176\cdot35} = \frac{1}{10\cdot9}$$

Dr. Hussey's observations were published for the purpose of exhibiting the advantage of a double-image micrometer over a wire-micrometer for measuring the diameters of planets. He at the same time measured *Jupiter*, and obtained for the ellipticity $\frac{1}{24{\cdot}9}$. It would appear, therefore, from this latter instance, that the tendency was to make the ellipticity too small rather than too large, since Struve's value, deduced from observations made with a wire-micrometer, is $\frac{1}{13{\cdot}7}$, and the mean of all the Greenwich observations with the double-image micrometer is between $\frac{1}{17}$ and $\frac{1}{18}$.

I will now proceed to a little farther discussion of my own measures, to obviate Mr. Dawes' objection that it is probable that the contacts for polar diameters may have been made by me too close on account of the dullness of the planet in their neighbourhood. For this purpose I will compare with the equatoreal diameters the means of the diameters measured at inclinations of 45° and 135°, since it is scarcely possible that the same objection can apply to these positions. They are reduced to the mean distance as follows :—

	Equat. Diam.	Mean of transverse Diam.
	″	″
1848, Sept. 20	18·53	17·51
22	18·75	17·58
Oct. 5	18·84	17·41
9	18·41	17·44
25	18·62	17·56
Nov. 15	18·18	17·59
21	18·65	17·75
1849, Jan. 26	18·83	18·13

It will be sufficiently accurate for our purpose, to take the mean of all the equatoreal diameters, and to compare it with the mean of all the transverse diameters. We get, then, for the former, 18″·60; and for the latter, 17″·62;

or, $a = 18{\cdot}60$

$r = 17{\cdot}62$ at inclination of 45°.

Now, generally $r = a\left(1 - \frac{\epsilon^2}{2}\sin^2\theta\right)$

where ϵ = excentricity $= \sqrt{1 - \frac{b^2}{a^2}}$

Hence, $17{\cdot}62 = 18{\cdot}60 - \frac{9{\cdot}30}{2} \times \epsilon^2$

or, $4{\cdot}65 \times \epsilon^2 = 0{\cdot}98$

whence $$\varepsilon^2 = 0{\cdot}211 = 1 - \frac{b^2}{a^2}$$

$$\therefore \quad \frac{b^2}{a^2} = 0{\cdot}789$$

and $$\frac{b}{a} = 0{\cdot}8882$$

Hence $$e = 1 - \frac{b}{a} = 0{\cdot}1118 = \frac{1}{8{\cdot}94}$$

The resulting ellipticity deduced from the transverse diameters, as compared with the equatoreal diameters, is, therefore, larger by a trifling quantity than that deduced from the polar diameters, but the agreement between this result, Bessel's result from measures of polar and equatoreal diameters when the ring had nearly vanished, and my own published result, is surprisingly close,—the published result being the *smallest* of the three.

From all these circumstances I cannot help considering the determination of the ellipticity of *Saturn*, from the mean of the above three results, to be as certain as that of any astronomical element whatever, heretofore given.

I would finally remark, that the figure of *Saturn* drawn with my ellipticity, corresponds well with the image of *Saturn* which I saw so frequently during the disappearance of the ring; but that if I trusted solely to my impressions from memory, I should feel inclined to dissent from the result as being too small rather than too large.

Extracts of a Letter from the Rev. Mr. Stoddart, an American Missionary, to Sir John Herschel, Bart., dated Oroomiah, Persia, N. Lat. 37° 28′ 18″, *Long. E. from Greenwich* 45° 5′, *Oct. 29th*, 1852.

"No one has ever travelled in this country without being surprised at the distinctness with which distant objects are seen. Mountains fifty, sixty, and even a hundred miles off, are projected with great sharpness of outline on the blue sky; and the snowy peak of Ararat, the venerable father of mountains, is just as bright and beautiful when two hundred miles distant, as when we stand near its base. This wonderful transparency of the atmosphere frequently deceives the inexperienced traveller; and the clump of trees, indicating a village, which seems to rise only two or three miles before him, he will be often as many hours in reaching.

"In this connexion you will be interested to know that the apparent convergence of the sun's rays, at a point diametrically opposite its disk, which, if I mistake not, Sir David Brewster speaks of as a very rare phenomenon, is here so common that not a week passes in summer, when the whole sky at sunset is not striped with ribbons, very much like the meridians on an artificial globe.

" But it is after nightfall that our sky appears in its highest brilliancy and beauty. Though accustomed to watch the heavens in different parts of the world, I have never seen anything like the splendour of a Persian summer evening. It is not too much to say that, were it not for the interference of the moon, we should have seventy-five nights in the three summer months, superior for purposes of observation to the very finest nights which favour the astronomer in the New World.* When I first came here I brought with me a six-foot Newtonian telescope of five inches aperture, of my own manufacture, and though the mirrors have since been much tarnished, and the instrument otherwise injured, its performance is incomparably superior to what it was in America. *Venus* sometimes shines with a light so dazzling, that at a distance of thirteen feet from the window I have distinguished the hands of a watch and even the letters of a book.

" Some few months since, having met with the statement that the satellites of *Jupiter* had been seen without a glass on Mount Etna, it occurred to me that I was in the most favourable circumstances possible for testing the power of the unassisted eye, and I determined at once to make some experiments on the subject. My attention was, of course, first turned to *Jupiter*, but for a considerable time with no success. It was always so bright, and shot out so many rays, that it seemed quite impossible to detect any of its moons, even at their greatest elongation from the planet. I varied the experiment in several ways, by looking through the tube of a small telescope, from which the lenses had been taken, and also by placing my eye near the corner of a building, so as to cut off the most brilliant rays of the planet, and yet leave the view unobstructed to the right hand or the left; but in neither case could I find any satellite. Sometime after I was sitting on the terrace, as daylight was fading into darkness, and thought I would watch *Jupiter* from its first distinct appearance till it shone out in its full splendour. This time I was exceedingly gratified just as stars of the first and second magnitude were beginning to appear, to see two, extremely faint points of light near the planet, which I felt sure were satellites. On pointing my telescope towards them, my first impressions were confirmed, and I almost leaped for joy. Since that night I have many times, at the same hour of the evening, had a similar view of these telescopic objects, and think I cannot be

* Some five or six years since, Professor Sestini, now of the Georgetown College, near Washington, U.S., made in Rome a series of observations on the colours of stars, and he has lately been engaged in comparing these colours as seen in Rome and in Georgetown, the same telescope being used in both cases; and to prevent deception, he has always made the American observations before consulting the observations made at Rome. He says,—" The results are remarkably uniform; with a few exceptions much more so than I anticipated. So that, were the state of the air to be ranked among the causes of the different colours of celestial objects, it seems to me that there is but little difference between this atmosphere and that of Italy, even with regard to transparency." From this I think I can infer that this climate must be superior for astronomical purposes to that of Italy.

mistaken as to the fact of their visibility. I must, however, add that none of my associates, who at my request have attended to the subject, are *sure* that they detect them, though the most sharp-sighted individual feels some confidence that he can do so. As these friends, however, are not practical observers, their failure to see the satellites does not shake at all my belief that I have seen them myself.

" The time during which these satellites are visible is hardly more than ten minutes. The planet itself soon becomes so bright that they are lost in its rays. I will not stop to discuss the question, in itself a most interesting one, why they are visible at all, when stars of the third and fourth magnitudes are not distinguishable, but merely give the facts in the case, knowing that you will reason on them far better than I can. Both the fixed stars and the planets shine here with a beautifully steady light, and there is little twinkling when they are forty degrees above the horizon.

" Having come to a satisfactory conclusion about the satellites of *Jupiter*, I turned next to *Saturn*. This planet rose so late in the night, that I had not seen it while watching *Jupiter*, and I was very curious to know whether any traces of a ring could be detected by the naked eye. To my surprise and delight, the moment I fixed my eyes steadily upon it, the elongation was very apparent, not like the satellites of *Jupiter*, at first suspected, *guessed* at, and then pretty clearly discernible, but such a view as was most convincing, and raised my wonder that I had never made the discovery before. I can only account for it from the fact that, though I have looked at the planet here with the telescope many times, I have never scrutinised it carefully with the naked eye. Several of my associates, whose attention I have since called to the planet, at once told me in which direction the longer axis of the ring lay, and that too without any previous knowledge of its position or acquaintance with each other's opinion. This independent collateral testimony is very satisfactory to me.

" I have somewhere seen it stated that in ancient works on astronomy, written long before the discovery of the telescope, *Saturn* is represented as of an *oblong* shape; and that it has puzzled astronomers much to account for it.* Am I not correct in this impression; and if so, is it not possible that here on these elevated and ancient plains, where shepherds thousands of years ago watched their flocks by night and studied the wonders of the glorious canopy over their heads, I have found a solution of the question?

" After examining *Saturn* I turned to *Venus*. The most I could determine with my naked eye was, that it shot out rays unequally, and appeared not to be round; but, on taking a dark

* We suspect that Mr. Stoddart labours under an erroneous impression on this point, the observations which ascribed an oblong shape to the planet having been *subsequent* to the discovery of the telescope. It is well known that when Galileo first surveyed the planet with his newly-invented instrument, he found, *to his great surprise*, that it was shaped like an olive.—R. G.

glass of just the right opacity, I saw the planet as a very minute, but beautifully defined crescent. To guard against deception, I turned the glass different ways and used different glasses, and always with the same pleasing result. It may be that *Venus* can be seen thus in England and elsewhere; but I have never heard of the experiment being tried.

"Let me say here that I find the naked eye superior for these purposes to a telescope formed of spectacle glasses, of six or eight magnifying power. This is not, perhaps, very wonderful, considering that in direct vision both eyes are used, without the straining of any one of the muscles around them, and without spherical or chromatic aberration or the interposition of a dense medium.

"As I am an entire stranger, and at the same time am desirous of having these statements make their full impression on your mind, it is proper for me to say that I was formerly for several years a pupil of Professor Olmsted, in New Haven, and have since been admitted to his special friendship: and that I was associated for some time in observations with young Mason, whose early death you have spoken of as a loss to the astronomical world; and though, no doubt, very many persons have more accurate habits of observation than myself, a practice of fifteen years has done much to train my eye for researches like these. You will also bear in mind the great dryness of our atmosphere, indicated by the register, as well as our great elevation. Captain Jacob (*Proceedings of the Edinburgh Royal Society*, vol. ii. No. 36), in speaking of the extinction of light in the atmosphere, says, 'the loss of light in passing from the zenith through a homogeneous atmosphere of 5·2 miles will be ·303. I was much astonished at first discovering that the air had so great absorbent powers, and many ideas are suggested by the fact.'

"My letter is already becoming tedious; but I will venture to trespass on your patience farther by naming a few test objects, which will enable you the better to compare the advantages of our position with your own.

"1. *δ Cephei.* This I have looked at repeatedly with my naked eye; and though I cannot be *sure* that I have seen it double, I put it down in astronomical language as 'strongly suspected.'

"2. The two small stars in the neighbourhood of the pole-star and in the general direction of *γ Cephei* are seen distinctly, and almost every night, as a single point of light.

"3. 4 and 5 *ε Lyræ* are very beautiful and well defined. When lying on my back, the view of these stars, as they have passed near the zenith, has been very similar to that I have often had of *Castor* in a good telescope. There being *no dew* here, it is almost the universal custom for the people to sleep upon the terraced roofs, which gives them a fine opportunity, if so disposed, to gaze on the blue vault above them.

"4. *α Libræ* is seen as two stars in any ordinary state of the atmosphere as readily as *α Capricorni* would be in America.

"5. *Mizar* and *Alcor* in *Ursæ Major.* On looking at these

any favourable night, two faint stars, which must be telescopic in England, are distinctly seen, one on each side of them."

On the Uranography of Saturn, including the Correction of certain Errors propagated generally in Scientific Works on that subject. By Dr. Lardner.

After noticing the views entertained by other writers upon this subject, the author enters into a mathematical investigation of the true form, magnitude, and position of the projection of the rings, as seen from different latitudes of the planet.

If we suppose the observer stationed at any given latitude and a perpendicular drawn from his station upon the plane of the rings, this perpendicular will be the sine of his latitude, while the distance of this perpendicular from the centre of the rings will be the cosine.

If the observer were stationed anywhere in the axis of the planet above the plane of the rings, a visual ray proceeding from his station to one of the edges, and supposed to curve round that edge, would describe a right cone, the axis of which would coincide with that of the planet. The projection of the edges of the ring upon the firmament would be the intersection of the surface of this right cone with the celestial sphere, and would obviously then be a parallel to the celestial equator, whose declination would be the complement of the semi-angle of the cone.

But if the station of the observer be transferred from the axis of the planet to any point on its surface, then the cone formed by the visual ray will no longer be a right cone; it will, on the contrary, be an oblique cone, the obliquity of whose axis to the plane of the rings will be equal to the latitude of the observer. The projection of the rings upon the celestial sphere will be the intersection of this oblique cone with the surface of the sphere, and will, therefore, no longer be a parallel of declination. Different points of its projection upon the sphere will have different declinations; that point which is upon the meridian of the observer above the horizon having the greatest, and that which is upon the meridian below the horizon having the least declination. All the analytical formulæ necessary for the determination of this projection of each of the edges of the rings upon the firmament are given in the paper, and such of the formulæ as are necessary for the purpose are tabulated for every two degrees of latitude within the limits of latitude at which the rings are visible.

It appears from this, that the rings form upon the firmament of the observer zones, the edges of which are at variable distances from the celestial equator, the point of each edge which crosses the meridian being most remote from the equator, and the distance decreasing from that point to the horizon; the point of the edge which is upon the horizon being, therefore, nearer to the celestial

equator, and having a less declination than any other point of the visible portion of the same edge.

From all this it follows that the several edges of the zones formed by the rings are intersected by parallels of declination at various angles, so that each parallel lying within the range of the rings is divided into two, three, or more segments, according to the number of edges which it intersects.

Objects in these parallels are carried by the diurnal motion of the firmament successively under the rings, are occulted by them, and are visible in the interval between them. Some objects are seen at rising below, and at setting above, both rings being visible between them while crossing the interval. The projection of the rings upon the firmament being symmetrical with relation to the meridian, prime vertical, and horizon, the same phenomena in each case, which are developed between the rising of an object and its culmination, are reproduced in a contrary order between its culmination and its setting.

An elaborate investigation of these phenomena relative to the fixed stars, the sun, the satellites, and the planets, are given in the paper; and the solar eclipses in particular, which take place during the winter half-year, are described with minute detail; and the continuance of each class of eclipses is given in terrestrial and Saturnian days.

Among the most remarkable and unexpected consequences of the principles laid down and developed in this paper, are the following:—

At all latitudes under 25° the rings permanently intercept no part of the firmament.

The effects of the diurnal motion, combined with the peculiar position of the rings, are such, that any object whatever which comes above the horizon passes at one time or another between its rising and setting, clear of the rings.

At latitudes between 25° and 45° a certain zone of the heavens, of small breadth, even when greatest, is permanently covered by the inner ring, so that all objects within this zone are perpetually concealed from the observer. This zone at 25° consists of a single parallel, and increases gradually in breadth.

At the latitude of 45° a similar zone begins to be permanently covered by the outer ring, and the width of this zone gradually increases with the latitude. Thus, between the latitude of 45° and that at which the rings wholly disappear by sinking below the horizon of the observer, the only portion of the heavens continually intercepted are two narrow zones, one covered by each of the rings, but less considerably in breadth than the projections of the rings; the breadth of these zones increasing, one from the latitude of 25° and the other from the latitude of 45° to the total disappearance of the rings.

When the sun, as it sometimes does, comes within the declinations which limit these zones of perpetual occultation, it is eclipsed continually from rising to setting.

This is the only case in which a continual solar eclipse is manifested, and this, as appears by the paper, only takes place for intervals of very limited duration and in particular latitudes. The most ordinary solar eclipses which are manifested are such as are of short duration, the sun being carried by the diurnal motion under one or other of the rings, or successively under both of them, and the duration of the eclipses being determined, not as assumed by Dr. Mädler, by the declination only, but by the diurnal motion and the conditions which determine the obliquity of the parallels of declination to the edges of the rings.

It is also shown that no zone of the heavens is constantly visible in the interval between the rings, except between the latitudes 56° and 58°, and there a zone bounded by two parallels very close together is alone visible. At all other latitudes the parallels of declination, which at one altitude pass between the rings, are at other altitudes covered by them.

None of the satellites, except the eighth, are ever eclipsed by the rings, except at or very near the Saturnian equator, where they are intercepted by the thickness of the rings. The eighth satellite, however, having an orbit inclined to the plane of the rings at about 13°, phenomena are developed in its monthly motion, which are analogous to the solar eclipses described above.

The thickness of the rings being unascertained, the author assumes its probable major and minor limits, and investigates the manner in which it would modify the phenomena.

To form a judgment of the interest and importance of this paper, it will, however, be necessary to refer to the paper itself, since it is rather on the details than any general views that the novelty and interest of the points developed in it depends.

Extract of a Letter from Sig. Secchi to the Astronomer Royal.

"I have the honour to announce to you the discovery of a new comet, found yesterday evening at $6^h\frac{1}{4}$ in the constellation *Lepus*, near the star μ. A first comparison with a small star of the 10th magnitude, which was very near the comet, showed that it was moving with rapidity towards the north. Here are the observations, made immediately afterwards by comparison with a star (a) of the 9th magnitude, with a ring micrometer on the equatoreal of Cauchoix.

1853.	Rome Mean Time.		
March 6	$8^h\ 55^m\ 42^s$	Comet's R.A. $= (a) + 7^s{\cdot}01$	Decl. $= (a) - 8'\ 14''$?
	$9\ 43\ 51$	— — $= (a) + 1^s{\cdot}07$	— $= (a) - 13\ 29{\cdot}7$

"The position of the star (a) has been obtained relatively to No. 9420 of the H.C. of Lalande, by the intermediate use of another star (e), and is as follows:—

App. R.A. (e)	$= 4^h\ 53^m\ 46^s{\cdot}32$	Decl. (e)	$= -16^\circ\ 19'\ 20''{\cdot}14$
— — (a)	$= 4\ 52\ 38{\cdot}77$	— (a)	$= -15\ 59\ 1{\cdot}38$

whence we conclude for the comet,

	Rome M.T. h m s	Comet's R.A. h m s	Comet's Decl. South. ° ′ ″
March 6	8 55 42	4 52 45·78	−15 50 47·2
	9 43 51	4 52 39·84	−15 45 41·7

Approximate Hourly Motion.

In R.A. = $7^s·4$ In Decl. 6′ 33″

"These observations are not corrected for the effects of refraction. The comet was a tolerably fine one. It had a large coma of about 5′, of which the brightest portion, of about 3′ in diameter, exhibited a small luminous point, which was taken for the point of observation: but it was not *too* distinct; it appeared even to have several of these points.*

"On the same evening there was found also a very feeble nebula in the constellation *Leo*. It follows the star γ by $3^m\ 34^s$, and is north of it by about 3′. It is not in the catalogue of Sir John Herschel. The two centres, or most luminous points, are separated by about 3^s in right ascension and by 3′ in declination, and connected by a feeble nebulosity."

"*Rome, March* 7, 1853."

The comet referred to in the foregoing letter was also discovered independently by Prof. Schweizer, at Moscow, on March 8, and by Dr. Hartwig, at Leipsic, on March 10. It was also discovered in America on March 8, as appears by the following message, which was telegraphed from Boston to New York, and transmitted to the Astronomer Royal by the steamer Arabia. The steamer, it is believed, was just on the point of sailing from the latter port when the message arrived.†

"*Boston, 9th March.*

"A comet was discovered at Harvard Observatory last evening, by C. W. Tattle. It is situated about five degrees south of the bright star *Rigel*."

M. D'Arrest, who has calculated the elements of this comet, remarks that they exhibit a distant resemblance to those of the comet of 1664. This would imply a period of about 188 years. It will readily be seen, by simple inspection of a Comet catalogue,

* This would seem to constitute the incipient symptom of a breaking up of the comet into several distinct parts, as in the case of Biela's comet. Hevelius remarks that a similar phenomenon was observed on the occasion of the apparition of the great comet of 1618. In the month of November, when the comet was first seen, the head presented the appearance of a nebulous mass of light. On the 20th of December it appeared to Cysatus at Rome to resemble a number of small stars (*Cometographia*, p. 417). Hevelius states that he himself observed a similar appearance in the head of the comet of 1661 and several other comets.—*Ib.* p. 341.—R. G.

† The original slip, as printed by the telegraph, was sent to the Royal Observatory.

that the agreement in this respect is *closer* than that which subsists between the elements of the new comet and those of any other comet whose orbit has been hitherto calculated. This is all which we are warranted in affirming in the present state of the subject. The question of identity, however, can only be *established* by a rigorous computation of the disturbing forces acting upon either of the two comets during the period comprised between their respective apparitions. The following comparison of the orbits of the two bodies will serve to exhibit the extent of their resemblance. The elements of the comet of 1664 are those given by M. D'Arrest. Both comets are referred to the mean equinox of 1853·0.

	Comet of 1664.	Comet (1) of 1853.
T	1664, Dec. 4·501 M.T. Par.	1853, Feb. 24·2797 M.T. Par.
	° ′	° ′ ″
ϖ	133 18	153 21 11·7
Ω	83 51	69 49 47·9
i	21 18	20 19 47·0
Log q	0·01104	0·038920
	Motion retrograde:	Motion retrograde.

The comet has been rapidly receding from the earth since its first apparition in Europe. When it passed its perihelion on the 24th of February, its declination *south* was 56° 32′. On the 30th of March, its declination *north* was 9° 22′. It would, consequently, have been very favourable for observation in the southern hemisphere. In all probability it has been observed at the Cape and other places on the southern side of the equator, several weeks previous to its apparition in Europe.

Reduction of the Observations of Eclipses of Jupiter's Satellites, Occultations of Stars by the Moon, and other Astronomical Phenomena, made by the late Rev. Thomas Catton, B.D., Fellow of St. John's College, Cambridge. By G. B. Airy, Esq., Astronomer Royal.

There is a preface to this paper by Mr. Airy, from which the following particulars are extracted.

The observations embrace a period of years extending from 1791 to 1832. The expense of their reduction, and of the printing of the results, has been defrayed from the fund placed by the Lords Commissioners of her Majesty's Treasury at the disposal of the Royal Society, for the aid of scientific enterprises undertaken by private individuals.

Mr. Catton's observatory was situate on one of the towers of St. John's College, Cambridge. Its exact position with respect to the Cambridge observatory was determined geodetically, previous to the reduction of the observations, by Professor Miller and Mr. Adams.

Mr. Catton's observations in right ascension were made with a

transit instrument of 3 feet 5 inches focal length, and $1^{in}.4$ aperture, executed by Sisson. The instrument was originally intended for a simple lens object-glass, but it is believed to have been fitted achromatically before Mr. Catton commenced his observations with it.

The clock had a gridiron pendulum. Its rate was found by Mr. Airy to be steady enough for the wants of extra-meridional observations.

The telescope used for observations out of the meridian appears to have been a 46-inch achromatic. It may be presumed that it had the usual aperture of $3\frac{3}{4}$ inches. An instrument (apparently a quadrant) was also occasionally used for taking altitudes or equal altitudes. It appears from the records of the observations that a divided object-glass was sometimes used for micrometric purposes.

The copy of observations furnished to the Astronomer Royal by Professor Miller consists of two parts.

The first contains meridional observations, principally with the transit instrument, together with notes on the intervals of the wires, the adjustments in collimation, level, and azimuth, &c., commencing with 1791, June 3, and ending on 1832, March 9. Sometimes (but rarely) there are nearly forty observations in one day; but there are numerous interruptions, extending to one or more months, and in one instance even to $2\frac{1}{2}$ years.

The second part contains observations of extra-meridional phenomena, eclipses of *Jupiter's* satellites, occultations of stars by the moon, solar eclipses, and transits of *Mercury*. The observations are accompanied with copious notes, which give a high opinion of Mr. Catton's care in observations. The first observation was on 1791, June 12, and the last on 1831, October 23.

It was evident that the only part of the observations which could possess any peculiar value are the extra-meridional. In the reduction, therefore, the results of which are exhibited in the above paper, the meridional observations have been considered as useful only for the determination of time.

There was exhibited at the meeting, a model of a mounting for a reflecting telescope of large dimensions, designed by the Astronomer Royal, who, at the close of the usual business of the meeting, gave an oral explanation of its construction.

It has been found to be desirable, in making observations with reflecting telescopes, that the same edge of the mirror should always be lowest. The main object of the contrivance proposed by Mr. Airy is to give an equatoreal motion to the telescope consistently with this condition.

The telescope is supposed to be 30 feet long, and to have a four-feet mirror and five-feet tube. It is intended that the mounting of the instrument shall satisfy the following conditions:—

1. The same edge of the mirror will always be lowest.

2. The motion of the telescope will be truly equatoreal.
3. The speed of the telescope for three hours on each side of the meridian will be truly equatoreal.
4. The micrometer-wire in the field of view will continue during the movement of the telescope to pass in the same direction through the same cluster of objects.

The first of these conditions is satisfied by giving the telescope a motion similar to that of an altitude and azimuth instrument. This object is accomplished by supporting the tube of the telescope by means of pivots or trunnions at its centre of gravity, on forks carried by a turn-table of a construction similar to that employed at railway stations. The turn-table is to turn with its centre bearing by a hollow cone upon a centre-pin, and with its circumference bearing upon six or nine wheels. Of these wheels, three will have their centres on firm bearings, and these will determine the position of the turn-table. All the others, as well as the centre-pin, will be carried on levers pressed upwards by springs or weights. The central pin will prevent horizontal unsteadiness.

The motion of the telescope is rendered equatoreal by bridling the elevated end of the tube to an upright pillar placed to the north of the instrument (supposing the place of observation to be in the northern hemisphere), the connexion of the tube with the pillar being such that an imaginary line joining the centre of motion in altitude and azimuth, with the point at which the bridling rod is attached to the pillar, is parallel to the axis of the earth's diurnal motion. The polar bridle is connected at the other end with an apparatus of small bars fixed to the mouth of the tube, and converging to the centre of the opening. It is adjustable in length, according to the polar distance of the object.

Placed to the east of the telescope, parallel to the imaginary polar axis above mentioned, is a real axis, which carries a quadrantal arc revolving in the plane of the equator; and also on the side of the quadrantal arc next the telescope, a graduated meridional arc, having a radial bar moveable along its face. The length of the radial bar is equal to the distance from the centre of motion of the telescope to the centre of the frame of small bars at the mouth of the tube. The radial bar is to be clamped on the meridional arc at the graduation corresponding to the polar distance of the object. The connexion between the telescope and the equatoreal quadrant revolving about the real axis is effected by means of a rod,—whose length is equal to the distance between the centre of motion of the telescope and the centre of the meridional arc,—uniting the upper end of the radial bar which moves upon the meridional arc, with the apparatus at the mouth of the tube. It is manifest from this arrangement that any increase or diminution of the equatoreal motion of the quadrant will be propagated in an equal degree to the tube of the telescope.

The speed of the telescope will, therefore, be rendered truly equatoreal by causing the quadrantal arc (the edge of which for this purpose is toothed) to be worked by a pinion placed on the

spindle of a clock-movement adjusted to sidereal time. When the telescope is in the meridian, the middle point of the quadrant is at the pinion. Hence it is evident that the range of the telescope will extend to three hours on each side of the meridian.

The fourth condition is satisfied by placing the small inclined mirror and the lateral eye-piece on a spindle passing upwards through the centre of the frame-work of small bars at the mouth of the tube, and connecting itself with the polar bridle. By this arrangement the spindle with the mirror and eye-piece are made to rotate within the tube, so that a line seen in the field of view of the telescope will always necessarily maintain the same position with respect to the meridian under view. The spindle is perforated, and when the small mirror is removed, an eye-piece for front view or a photographic plate may be inserted in the perforation.

With respect to an objection which might be urged against this contrivance, in consequence of the obstruction offered to the passage of the light by the apparatus at the mouth of the tube, Mr. Airy remarked that no serious inconvenience would arise from this cause, since the bars composing the apparatus might be constructed of such small dimensions as to occasion only a very trifling loss of light. Nor could the diffraction of light produce any sensible effect, since it is a well-established fact that the influence of this physical principle diminishes continually as the aperture of the telescope is increased.

An objection, which seems to be better founded than either of the above, was offered to this mode of mounting, on the ground that it would be impracticable with such a contrivance to make observations near the zenith. This is a difficulty which presents itself in every form of mounting combining equatoreal movement with an arrangement that keeps the same edge of the mirror downwards. When the question, however, refers, as it does in the present case, to a large reflecting telescope, which is designed mainly for the purpose of making *physical* observations of the celestial bodies, it does not appear that this circumstance is calculated materially to impair the practical utility of the instrument.

It will readily be seen from the foregoing description, that one of the leading features of the mounting proposed in the present instance, consists in the adaptation of its construction to the principles by which the engineer is guided in giving stability to his mechanical arrangements, and in properly adjusting the pressure upon their various points of support. The recent history of the Greenwich Observatory has amply demonstrated, that when instruments of large size are constructed with a due regard to the strains which their different parts have to sustain, their working becomes as practicable as that of smaller instruments, while the advantages arising from such a combination of magnitude, stability, and equable wearing, vastly outweigh those derivable from mere artistic excellence.

It may be mentioned that the model still remains for inspection at the apartments of the Society.

On Four small Stars which had been observed during the preparation of Mr. Bishop's Ecliptical Charts, and have since disappeared. By Mr. Hind.

"In the course of forming our ecliptical charts I have seen, and subsequently lost, four small planets which remained to be rediscovered. I will briefly give the particulars.

"The first occurred upon our chart for 3^h right ascension, and is actually printed on the map. Its position for 1825 is, R.A. $3^h\ 1^m\ 40^s$, N.P.D. 71° 16′, and it was observed between the 8th and 11th of February, 1851, while I was finishing off the map for the engraver. By some oversight I did not discover its disappearance from the above place until August following, when it was useless to search after it.

"The second planet was missed, 1852, Oct. 11, having been entered in the previous spring in R.A. $7^h\ 46^m\ 55^s$, N.P.D. 68° 14′ 5″, for the epoch of the map (1825). It was of the 11th magnitude, and like the last has appeared by an oversight upon the published chart. Unfortunately, I cannot be quite sure of the date when this object was entered. It was certainly between 1852, February 25, and the middle of April; most probably about the beginning of the latter month. The third missing star, which I presume to have been also of a planetary character, was marked on October 25, 1852, in R.A. $3^h\ 23^m\ 40^s$, N.P.D. 68° 30′, near another of the same (eleventh) magnitude upon the map for that hour of right ascension. I missed it on December 8, and subsequently made several fruitless attempts to rediscover it; the want of consecutive fine nights, however, was against my finding the planet again. I suspect it moved away to the north, and that the inclination must have been rather large.

"The fourth object was missed from the chart, Hour IV., last month. I cannot at present recover the date of entry, though it must have fallen during the first eight months of 1852. It was of the eleventh magnitude, and appears upon the published map in R.A. $4^h\ 5^m\ 25^s$, N.P.D. 66° 40′. Did I not know from experience how easy it is to omit noticing the absence of a small star from its place upon the map, I might be inclined to consider the second and fourth as variable stars; the first and third I am satisfied were planets, and the other two I think were probably planets also."

"*Mr. Bishop's Observatory, Feb.* 11, 1853."

The American Ephemeris and Nautical Almanac for the Year 1855. Published by authority of the Secretary of the Navy. (Pp. 474, including an Appendix, pp. 78, royal 8vo.) Washington, 1852.

A fresh proof of the interest felt by our Transatlantic brethren in the advancement of astronomical science, and of their ability in carrying out views of practical utility, has recently been afforded by the publication of the handsome volume above entitled. The

preparation of this work was begun in the latter part of the year 1849, in pursuance of an act of Congress approved on the 3d of March of that year. Lieut. Davies, U.S. Navy, has been appointed Superintendent of the Ephemeris. The theoretical part has been placed under the direction of Prof. Peirce.

The work is divided into two distinct parts. The first part is appropriated to nautical purposes, and is calculated for the meridian of Greenwich. The second part is designed for the promotion of astronomical science, and is adapted to the meridian of Washington.

The nautical part consists of an ephemeris of the Sun and Moon and of the planets *Venus*, *Mars*, *Jupiter*, and *Saturn*, together with tables of Lunar Distances. There are also appended the rectangular co-ordinates used in computing the ephemeris of the Sun.

The ephemeris adapted to each month of the year is comprised in *sixteen* pages.

Page i. is devoted to the Sun. The computations are for *apparent noon*, and the co-ordinates are exclusively those of *right ascension* and *declination*.

Page ii. contains the tables of the semidiameter, horizontal parallax, and time of meridian passage of the Moon. There are columns exhibiting the variations of the horizontal parallax in one hour, at noon and midnight, and also a similar column for the variation of the time of meridian passage.

Pages iii.–x. contain the apparent right ascension and declination of the Moon for every hour of each day of the month, with columns of differences for one minute.

Pages xi.–xvi. are devoted to the Lunar Distances. The arrangement is similar to that employed in the *Nautical Almanac*.

The planetary ephemeris contains the apparent right ascension and declination at Greenwich noon of each of the four planets above mentioned, with the variations of both co-ordinates for one hour; also the time of meridian passage.

The following remarks have been suggested by an examination of the second part of the work.

The logarithms of the quantities A, B, C, D, used in correcting the places of the stars are computed to *five* places of decimals.

The values of the constants of Aberration, Precession, and Nutation employed in these computations are those assigned by MM. Peters and Struve.

The mean places of 100 principal fixed stars are given. These have been derived either from Airy's twelve-year Catalogue of 2156 stars or from the Catalogue of the British Association.

The tables of the apparent places of the principal stars for the upper transit at Washington are arranged similarly to the corresponding tables of the *Nautical Almanac*.

The solar ephemeris is computed both for *mean* and *apparent* noon.

The ephemeris of the Moon's place contains the apparent right ascension and declination at the upper and lower culminations,

with the requisite columns for reduction. There is also a column with numbers for reference to the various stars which accompany the successive culminations of the Moon. A separate list of all the moon-culminating stars is inserted immediately after this ephemeris.

Tables are given for computing the Libration of the Moon, both in longitude and latitude.

The ephemeris of *Mercury* contains the apparent right ascension and declination at mean noon and transit, with corresponding logarithms for interpolation; also the mean solar time of meridian transit.

The ephemeris of *Venus* is similar to that of *Mercury*, except that it exhibits the logarithms of motion in a sidereal minute both in right ascension and declination, with the logarithms of the factors for second differences.

The ephemeris of each of the remaining planets is similar to that of *Venus*, except that the apparent right ascension and declination are calculated for sidereal noon and for *transit*.

There is no ephemeris given of any of the small planets revolving between *Mars* and *Jupiter*.

The rectangular co-ordinates of the Sun are given for every twelve hours, with the corrections for reducing them to the mean equinox of 1855·0. The true longitude, the latitude, and the logarithm of the radius vector, are also computed for the same intervals.

There are tables of the heliocentric co-ordinates of the planets. The table for each planet contains the rectangular co-ordinates, the logarithm of the radius vector, and the longitude of the planet in its orbit. The tables of *Mercury* and *Venus* are calculated for every five days; those of *Mars* and *Jupiter* for every ten days; those of *Saturn* and *Uranus* for every twenty days; and those of *Neptune* for every forty days.

The ephemeris of the Moon was calculated from Plana's theory, combined with Hansen's inequalities due to the disturbing action of *Venus*, Airy's corrections of the lunar elements, and Hansen's secular inequalities of the mean motion of the lunar perigee. An empirical correction by Mr. Longstreth, of Philadelphia, was also used.

The ephemeris of *Mercury* was derived from the theory of the planet given by Le Verrier in the *Connaissance des Temps* for 1848. The ephemeris of *Venus* was founded on Lindenau's tables, applying Mr. Hugh Breen's corrections of the elements (*Mem. Ast. Soc.* vol. xviii.); Airy's Long Inequality (*Phil. Trans.* 1832); and Le Verrier's secular variations of the elements (*Connaissance des Temps*, 1841). The ephemeris of *Uranus* was calculated from Bouvard's ellipse, combined with Le Verrier's perturbations by *Jupiter* and *Saturn* (*Connaissance des Temps*, 1849); and Peirce's corrections of the elements and perturbations due to *Neptune*. The ephemeris of *Neptune* was founded on Walker's orbit and Peirce's theory of its perturbations. In other respects the data of the planetary ephemeris agree very nearly with those adopted in the *Nautical Almanac*.

There are inserted in this part of the work the elements of the solar and lunar eclipses which will happen in the year 1855. Tables are also given, by means of which the solar eclipses may be accurately computed for any assigned place. These tables are founded on a modification of Bessel's formulæ suggested by Mr. T. Safford, jun.

Tables are given for calculating the occultations of all the stars down to the *sixth magnitude*, contained in the Catalogue of the British Association, which can be occulted during the year 1855. The method of Bessel forms the basis of computation.

In addition to the usual list of eclipses, &c. of *Jupiter's* satellites, a complete set of tables is given, by the aid of which the configuration of the satellites corresponding to *any assignable time* may be readily ascertained.

There is a table by Dr. B. A. Gould, containing the geographical positions of the principal observatories which are at present in astronomical activity, or which have been so within the last quarter of a century. The latest and most trustworthy determinations have been employed in its preparation. The following is the position assigned to the Washington Observatory:—N. Lat. $38^\circ\ 53'\ 39''{\cdot}25$ (*Ast. Journal*, iii. 12); Long. W. from Greenwich, as derived from data of the U.S. Coast Survey up to 1852, $5^h\ 8^m\ 11^s{\cdot}2$.

The Appendix, in addition to other matters of less importance, contains Chauvenet's tables for correcting lunar distances and an ephemeris of *Neptune* for the year 1853.

Various individuals have co-operated in the computation of this Ephemeris. The results have in most instances passed under the final revision of Prof. Peirce.

It may be remarked, in conclusion, that the work is beautifully got up. The order of arrangement is neat and lucid; the printing is in a clear bold type, and the paper is of superior quality. The modern style of numerals is used throughout, apparently without any disadvantage.

Upon the probable Identity of the Solar Faculæ with the rose-coloured Protuberances seen during a Total Eclipse of the Sun. By M. Schweizer, Astronomer of the Institute of Geodesy, Moscow. (*Ast. Nach.* No. 849, p. 133.)

In an account of the total eclipse of the sun of August 8, 1850, observed at Honululu, M. Arago threw out a conjecture relative to the possible identity of the solar faculæ with the red protuberances seen around the margin of the moon during the occurrence of the totality.* M. Schweizer was induced by this circumstance to institute a series of observations of the solar faculæ about the time of the total eclipse of July 28, 1851, and to cause drawings of their appearances to be carefully executed by some of the most

* *Comptes Rendus*, April 21, 1851.

expert pupils of the Institute.* The observations were made with a Fraunhofer instrument of 3·3 inches aperture, and a magnifying power of 55. They were commenced on the 9th of July, and were continued till some time after the occurrence of the eclipse. Of the whole series of drawings, a select set, amounting to twelve in number, and representing the appearances of the faculæ during the few days which elapsed before and after the eclipse, were lithographed for the purpose of wider diffusion. A copy of these drawings accompanies the number of the *Astronomische Nachrichten*, containing M. Schweizer's communication. By comparing these drawings with the accounts of the protuberances seen during the total eclipse of July 28, M. Schweizer shows that a remarkable analogy exists between the latter phenomena and those to which the drawings relate. Thus, with respect to the hook-shaped protuberance, seen by all observers of the eclipse on the western limb of the sun, at a distance of about 279° from the north pole of the solar disk, counting towards the east, there was found upon the drawings a *similarly-formed* facula, having the *same position*, which continued from the morning of the 26th of July, when it was first seen, to approach nearer and nearer the margin of the sun's disk, and *had already quitted it on the 28th*, in virtue of the sun's rotation. An equally satisfactory agreement was found to present itself upon comparing the drawings of the faculæ with the descriptions of the isolated red patch seen in the vicinity of the hooked protuberance, and also in several other similar instances. M. Schweizer sums up the results of his comparison in the following terms:—

1. For every group of faculæ which appeared on the *western* margin of the sun's disk within two days *before* the eclipse, and for every group which appeared on the *eastern* margin within a similarly short time *after* the eclipse, and which were demonstrated by the drawings not to be on the sun's disk on the 28th of July, corresponding protuberances were seen.

2. Notwithstanding the rather sudden changes of form to which faculæ are subject, still there were several of them which presented a striking resemblance in this respect to the corresponding protuberances.

3. On the western border of the sun the *configuration* of a hooked, a round, and an elongated protuberance, was exactly the same as that of the corresponding and similarly-formed faculæ.

M. Schweizer remarks, that it is difficult to suppose so striking a coincidence to be purely accidental, and that the probability rather is, that the protuberances are no other than the faculæ which we so often see upon the sun's disk. This conclusion might be objected to by some persons, on the ground that the light of the faculæ is generally more intense than that of the other parts of the solar surface, while, on the other hand, the protuberances shine only by a moderate red light. M. Schweizer, however, seems to

* M. Schweizer states that the pupils of the Institute are habitually instructed in drawing from a very early age.

think that there are circumstances connected with the appearance of the faculæ which serve to obviate this objection.

First, he asserts it to be a well-known fact, that even bright groups of faculæ, as they advance from the eastern border of the sun towards the middle of the disk, now and then vanish, and even seem to reappear in altered forms after they have passed the middle parts of the disk. Again he states, that both himself and his assistants have remarked that the faculæ, although occasionally quite visible when they are *just upon the edge of the sun's disk*, still are, at other times, much less conspicuous in that position than at *some distance* from the edge. He estimates the maximum brightness of the faculæ to be in those parts of the disk which are situate at a distance of from $\frac{1}{2}'$ to $5'$ or $6'$ from the margin. Hence it would appear, that the brightness of the faculæ depends on the position which they occupy with respect to the imaginary line uniting the centre of the sun and the eye of the observer. "Might it not then be easily possible," he says, "that as, in the middle of the sun's disk, so also upon its margin, where the radius forms a right angle with the line before-mentioned, the light of the faculæ should not surpass the rest of the solar surface? nay, might it not descend in intensity to the light of the protuberances? The light of the latter cannot be so inconsiderable, since they continued to be seen, according to some accounts of the eclipse, for a considerable time after the reappearance of the sun.* If this be really the case, it is probable that the cloud-like faculæ merely represent transparent objects, in a small degree or even not at all self-luminous, which allow the sun's light to pass in certain positions, with respect to the eye of the observer, without being themselves visible; in other positions, to transmit to us a maximum of the solar light, either by refraction or reflexion, with such intensity that they appear brighter than the surrounding parts of the surface; and finally, in other positions, to appear with the mild light and colour of the protuberances."

Without taking into account the impossibility of a mere accidental coincidence of the drawings of the faculæ with the protuberances, M. Schweizer sought to establish a connexion between the latter and the penumbræ of the solar spots; but he was unable to arrive at any consistent results. During the days immediately preceding and following the eclipse the number of such penumbræ visible upon the disk was too small to account for the numerous protuberances, and even of those penumbræ or nuclei which *did* appear, not one, however small, was situate *exactly upon the margin of the disk*, as ought to have resulted if they had been identical with the protuberances, and as really occurred in the case of the faculæ. Moreover, the forms of the protuberances do not in anywise resemble those of the penumbræ seen near the margin of the solar disk; while, on the other hand, any person who has observed the

* In Mr. Dawes' account of the eclipse there occurs the following remarkable statement relative to the hooked protuberance:—"To my great astonishment, this marvellous object continued visible for about 5^s, as nearly as I could judge, after the sun began to reappear." (*Monthly Notice*, vol. xii.; *Memoirs*, vol. xxi.)—R. G.

solar faculæ, and has seen M. Fearnley's admirable drawing of the total eclipse, cannot fail to be struck with the resemblance between the forms of the protuberances and those of the faculæ, more especially in the case of the vein-like shoots which gradually lose themselves in the surface of the disk.

In No. 849 of the *Astronomische Nachrichten* there appear observations by Mr. Hind of two variable small stars, which he discovered at Mr. Bishop's observatory in the year 1848. The two stars are situate very near to each other. The following are their positions :—

	R.A.	N.P.D.	
	h m s	° ′ ″	
Preceding Star	7 34 2	66 12 2	(1850)
Following Star	7 40 18	65 53 8	

Mr. Hind concludes from his observations that the preceding star was at a maximum (though not so bright as in previous years) about the middle of December 1852, and that the following star was also at a maximum towards the middle of November. He estimates the periods at 280 and 290 days respectively, but he is of opinion that all the observations he has hitherto made are hardly sufficient to settle them within several days.

THETIS.

M. Schoenfeld, of Bonn, has investigated new elements of this planet, and has employed the results in computing an ephemeris for the opposition of 1853, which extends from May 16 to September 21 (*Ast. Nach.* No. 848, p. 119).

COMET (I.) 1850.

For observations of this comet made at the Cape of Good Hope by Mr. Maclear, between September 6 and October 15, 1850, see *Ast. Nach.* No. 852, p. 181.

IRENE.

M. Trattenero, of Padua, has determined new elements of this planet, and has calculated an ephemeris for the opposition of 1853, extending from October 1, 1853, to January 20, 1854 (*Ast. Nach.* No. 852, p. 191).

ERRATA.

Page 70, line 14, *for* R^2, *read* K^2.
— —, — 27, *for* $\sqrt{\frac{1}{3}K^2 - M^2}$, *read* $\sqrt{\frac{1}{4}K^4 - M^2}$.
— 93, — 29, *dele* Sir.
— 97, — 2, *for* Ross, *read* Roy.
— 126, — 36, *for* them, *read* three.

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